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ABSTRACT

Recently, a number of evaluations of private school voucher programs in the United States have reported achievement gains for voucher users, especially African Americans. These studies tend to be structured as Randomized Field Trials (RFTs), where participants are assigned to treatment (offered a voucher) and control (not offered a voucher) groups by lottery. A major advantage of RFTs is that the randomization process controls for a number of factors, measurable and unmeasurable, that otherwise might confound the assessment of voucher effects. A major shortcoming of RFTs is that they tend to be black-box evaluations that inform policy analysts little about why or how a policy intervention yields benefits downstream. Data are used from the second year RFT of the District of Columbia privately funded voucher program, supplemented by information obtained from the schools that participating students attended, to identify what school features or practices might be boosting the achievement of voucher students. This preliminary analysis suggests that especially dedicated teachers, a higher proportion of white and higher-income students, and more demanding homework assignments may be the characteristics of private schools that increase the academic achievement of inner-city school voucher users. Additional analyses are recommended before firm conclusions are drawn regarding what happens inside the black box of school voucher experiments. Appended are variables used in the analysis. (Contains 7 tables and 25 references.) (Author/BT)

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Looking Inside the Black Box: What School Factors Explain Voucher Gains in Washington, D.C.?

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Abstract

Recently, a number of evaluations of private school voucher programs in the United States have reported achievement gains for voucher users, especially if they are African American. These studies tend to be structured as Randomized Field Trials (RFTs), where participants are assigned to treatment (offered a voucher) and control (not offered a voucher) groups by lottery. A major advantage of RFTs is that the randomization process controls for a number of factors, measurable and unmeasurable, which otherwise might confound the assessment of voucher effects. A major shortcoming of RFTs is that they tend to be "black-box" evaluations that tell policy analysts precious little about why or how a policy intervention yields benefits downstream. Here we use data from the second year RFT of the Washington, D.C. privately-funded voucher program, supplemented by information obtained from the schools that participating students attended, in an effort to identify what school features or practices might be boosting the achievement of voucher students. This preliminary analysis suggests that especially dedicated teachers, a higher proportion of white and higher-income students, and more demanding homework assignments may be the characteristics of private schools that increase the academic achievement of inner-city school voucher users. Additional analyses are recommended before firm conclusions are drawn regarding what happens inside the black box of school voucher experiments.

I. Introduction

Recently a number of major studies have been conducted on the effects of school vouchers on the educational achievement of students (Howell et al 2002, Greene 2001, Peterson et al 1999). These studies tend to identify positive academic effects of vouchers on the African-American students who use them to switch from public to private school. The studies are designed as Randomized Field Trials (RFTs), with families assigned to treatment and control groups by lottery. The randomization process tends to control for all other factors, besides the treatment, that otherwise might confound the analysis. Therefore, analysts can ascribe to the treatment responsibility for any post-intervention differences between the students who received vouchers and the control group who did not. Economist Carolyn Hoxby has described this methodological approach as the "gold standard" for research on educational interventions, such as vouchers, that otherwise would be vulnerable to self-selection bias (Hoxby 2000a).

In spite of the advantages of these RFTs for correctly identifying the independent effects of school voucher interventions, they have at least one significant shortcoming: by themselves they do not indicate why vouchers improve the educational performance of the students who use them. Randomized Field Trials are classic "black-box" analyses, where a condition is measured pre-test, the treatment is administered to one randomly determined group but not another group, and a post-test is administered. Participants are baseline tested, one group is permitted to enter the black box, and the groups are re-tested after the treatment group emerges from the black box. What happens inside the box in order to generate post-test differences in the two groups is not the focus of such studies.

This agnosticism regarding the proximate causes of school voucher gains prevents the RFTs from being maximally informative regarding policy reforms. If voucher students learn

more simply because their private schools have smaller class sizes, more elaborate facilities, or assign more homework, then such beneficial traits could be adopted by the public schools that serve the low-income inner-city populations who are eligible for vouchers, thus obviating the need for them to seek a private school education. Moreover, if voucher gains are simply due to the benefits that low-income students receive from interacting with higher income or more racially diverse peers, then voucher effects might diminish as more low-income African-American students take advantage of such programs (Fisk and Ladd 2000). There is simply a finite pool of higher income peers available to mix with lower income students, whether one lives in New Zeeland or Lake Wobegone.

As a result, it is critically important for us to try to discern what is happening inside the black box of voucher interventions that is making a positive difference for low-income inner-city students. This paper puts forward the initial baby-steps for such a journey. Using the data from the second year evaluation of the Washington Scholarship Fund school voucher program, we attempt to identify one or more particular characteristics of schools that represent proximate causes for the greater academic achievement of voucher students. Our analysis is essentially exploratory and our findings are highly preliminary. They suggest that there is no single silver bullet characteristic that individually explains voucher gains. However, it appears that greater school resources, smaller schools, smaller class sizes, order and discipline, and a stronger sense of community *do not* explain the voucher gains. The best candidates for factors that *are* driving the educational improvement of voucher users are more racially diverse and economically advantaged peers, more homework, and dedicated teachers. More research will be necessary in order to confirm that these initial findings are reliable.

II. Theory

The established theories regarding which school characteristics improve the performance of low-income inner-city students are both rich and varied. They include claims that school resources, size, community, order and discipline, high expectations, and student body demographics are the reasons why some disadvantaged urban students achieve more than others do. We will consider these theories in turn.

A. Resources

Entire rain forests have been felled in the debate over whether or not increased resources measurably contribute to positive educational outcomes. Politicians certainly behave as if they believe that more resources purchase better education, as per-pupil spending on education has more than quintupled, in real terms, over the past fifty years (Brandl 1998, 2-3). Moreover, educational equity in America has often been defined in terms of the equalization of spending on students across local school districts and states, with courts intervening to order such equalization (Reed 2001). Eric Hanushek (1996, 1989), among others (e.g. Brandl 1998), has argued repeatedly that additional investments of resources in K-12 education, in general, fail to produce consistent payoffs in the form of higher student achievement. Other scholars have adopted the moderate position that more money improves educational outcomes, even dramatically, when it is invested properly (Hedges and Greenwald 1996, Hedges et al 1994). Such a claim could be fully consistent with Hanushek's findings, if it is the case that most schools do not invest additional resources in the sorts of products and activities that yield positive achievement benefits for students. The investment problem might be particularly acute for the inner-city schools thattend to serve the populations who are eligible for vo uchers, as they

may face more claims for, less discretion regarding, and less expertise in the effective deployment of resources.

B. Scale

Throughout the Twentieth Century, a dominant maxim regarding K-12 education was that bigger is better. Large elementary and secondary schools could take advantage of economies of scale to provide better facilities and more diverse academic programs and extra-curricular activities to their larger student bodies. However, recently there has been a movement towards a "smaller is better" mentality. Smaller schools, it is thought, are less imposing to students and better able to provide them with individual attention (Nathan and Febey 2001). Smallness appears to be most important in the classroom, as two influential studies of a class-size reduction experiment in Tennessee determined that smaller classes enhance student performance, at least in the younger grades (Mosteller 1995; Krueger 1999).

C. Community

Schools serve disadvantaged students best when they function not as educational bureaucracies or businesses but as educational communities. Noted education analysts James Coleman and Thomas Hoffer (1987), John Brandl (1998) and Anthony Bryk and his colleagues (1992) have all argued that students, especially those from disadvantaged backgrounds, thrive in private schools because of social capital that is produced by the nurturing of a strong sense of community. Private schools communicate more with parents and draw them into the school to participate, in a partnership, in the education of their child. Teachers have a strong sense of mission that is driven by concerns for the well-being of each student. It is precisely this fostering

of education as a communal enterprise that results in disadvantaged students learning more in private schools, they argue.

D. Order and Discipline

Disadvantaged inner-city students are more likely to face disorder on a daily basis—in their neighborhoods, schools, and homes. The danger and unpredictability of chaotic environments naturally engenders fear and hopelessness in young people. Schools that are able to establish a safe and well-ordered environment, through strict discipline and careful monitoring of the school and classrooms, likely serve as a firebreak to hopelessness and fear for low-income urban children. Moreover, disorder in the classroom distracts teachers from teaching and students from learning. It is generally recognized that an orderly learning environment is a necessary pre-condition for effective learning in school (e.g. Wilson 1989, 21-23).

E. Homework

A number of educational studies have demonstrated that students tend to live up to the high or low expectations that are set for them by parents and teachers. Ferguson (1998) argues that expectations are at least partly to blame for the infamous "black-white test score gap" of nearly two years in the achievement of comparable black and white students by the time that they are seniors in high schools. Less is expected of disadvantaged students in terms of educational achievement, and they tend to deliver only what is expected of them. If even disadvantaged students are assigned challenging homework and high expectations for success in completing it, progress might be made in closing the test score gap.

F. Peers

A rich theoretical and empirical literature has established that characteristics of a student's peer group have independent and significant effects on the student's academic achievement (Nielsen and Wolf 2001; Hess and Leal 1997; Hoxby 2000b). Since the seminal Supreme Court ruling *Brown v. Topeka Board of Education*, our society has operated under the maxim that an educational system that is entirely separated by race is inherently unequal to the minority students involved. Thus, racial desegregation has long been thought to be an important vehicle for improving educational outcomes for low-income African American students. All other things equal, we would expect disadvantaged minority students to learn more if they were to be educated in a racially diverse environment. We also would expect lower-income students to perform better academically if they were educated with higher-income students as their peers.

All of the above theories are possible candidates for explaining what happens inside the black box to produce voucher gains. However, even this extended list of theories fails to entirely exhaust the possible explanations. Another peer group factor, the average academic ability of the student body at the schools, could not be measured consistently, since different schools use different tests and performance standards. The leadership of school principals and the quality of teachers may be driving voucher gains. Since we were unable to obtain direct measures of those and other classroom factors, we may not be able to identify the proximate cause behind voucher gains. Still, we should be able to shed some light on the question. We now turn to the data we will analyze in order to preliminarily rule in or out each of these competing explanations.

III. Data and Methods

A. Data

The core of the data that we use in the study comes from the second year evaluation of the Washington Scholarship Fund (WSF) privately-funded voucher program. The WSF provides partial tuition scholarships of up to $2,200 to families in the District of Columbia with household income at or below 270% of the federal poverty line. Families with income below the poverty line are eligible for the maximum scholarship amount, whereas families at 270% of poverty are eligible for about half of the maximum. The scholarships are like vouchers, which can be redeemed at any of the more than 100 D.C. private schools that participate in the scholarship program. WSF has been awarding scholarships to D.C. students since 1993. Currently, the WSF supports 1,325 elementary and secondary school students with scholarships.

The WSF experienced a dramatic expansion of their scholarship program in 1998. Since demand greatly exceeded even the supply of 1,000 new scholarships, the vouchers were awarded by lottery. Because only the luck of the draw determined which family would or would not receive a voucher, the effect of the voucher on student and family outcomes could be studied via a Randomized Field Trial (RFT). In the spring of 1998, before the scholarships were awarded, the families of 1,582 students were surveyed about their family characteristics and school experiences. The students, who all were enrolled in grades 1-8 of a public school at the time, completed the Iowa Test of Basic Skills in reading and math in order to produce a baseline measure of their academic abilities. After baseline data collection was complete, 811 of the students in the study population were awarded scholarships by lottery. The remaining 771 students comprised the control group for the study. The treatment and control groups in the D.C.

evaluation did not differ significantly on any of the nearly 50 characteristics (Wolf, Howell and Peterson 2000). The lottery appears to have worked.

Two years after the voucher offer, in the spring of 2000, the remaining members of the treatment and control groups were invited to data collection sessions in which their children were re-tested and the parents and older students were again surveyed about their educational experiences. Since 125 members of the control group had won the turnout incentive lottery in the first re-test year of 1999, a total of 1,457 students remained in the study population. The second-year turnout of 730 students comprised 50% of that population, for both the treatment and control groups.

The second-year WSF evaluation data were generated via a number of measurement instruments. The baseline and second-year test scores were generated by the performance of students on the ITBS in 1998 and 2000, respectively. Parental reports of school factors such as indicators of school community and order were obtained from survey instruments. The students in grades 4-9 were surveyed about such things as teacher attitudes, homework, and strict discipline at their schools (Wolf, Peterson and West 2001).

These core data from the RFT were then supplemented by information collected from and about the various public and private schools that the students attended during the 1999-2000 academic year. The supplemental data included information about per pupil spending on students, as well as statistics regarding student body characteristics, enrollments, and class sizes. For the non-charter D.C. public schools in the sample, these data were provided to us by the district's Office of Public Accountability. For the private schools in the sample, the data were obtained from two sources, depending on the type of private school involved. Information about the Catholic parochial schools in the sample were obtained from the Office of the Superintendent

of Schools for the Catholic Archdiocese of Washington, D.C. Statistics regarding the non-parochial religious and independent private schools in the sample were obtained from responses to a survey we mailed to those schools. The response to the mail survey was high, thanks in part to our persistence in following up with private school administrators, as we obtained at least some information from over 80% of the private schools in the sample.

Still, moderate data gaps remain, especially regarding potentially sensitive subjects such as the racial and income demographics of the schools. To prevent non-random list-wise deletion of observations from imperiling both the validity and efficiency of the analysis, we replaced missing data in our explanatory variables with 0 values and included missing data dummy variables in the analysis. By replacing the missing values, we avoided the inefficiency and potential bias caused by the exclusion of a significant proportion of the observations. By including a missing data dummy variable for any variable so altered we prevent the replacement zeroes from biasing the estimation of the beta coefficients in the statistical model (Cohen and Cohen 1983, pp. 275-300). Still, including a significant number of such non-theoretical covariates reduces the efficiency of the estimations somewhat (i.e. burning one degree of freedom per missing data variable) and potentialy biases estimates if the missing data dummies co-vary significantly with theoretically-based explanatory variables (Peterson and Howell 2003). To minimize the likelihood of such problems, each theoretical claim regarding the proximate causes of voucher gains was tested in a separate, parsimonious statistical model.

Table A in the Appendix presents descriptive statistics regarding the variables used in the analysis. Where the operational measurement of the variable is not obvious, a description is provided in the notes to the table. As revealed by the numbers in the far right column, the most significant missing data problem comes from measures such as the "Caring Teaching Index"

which are constructed from student survey responses. That is because the third graders in the study were too young to be surveyed, and some of the students in the older grades who were surveyed left questions blank. Several of the variables that rely upon data drawn from the schools themselves also suffer from significant data gaps, especially those for student body demographics. Still, a reasonably large pool of actual data is available to estimate the relationships discussed in this paper.

B. Methods

In this paper, we report on the effects of private schooling on student test scores, also known as the effect of treatment on the treated. Even within the context of an RFT, such estimates can suffer from bias, since some of the participants who are randomly assigned to the treatment fail to use it (i.e. remain in public school) and some participants who are randomly denied the treatment obtain it anyway (i.e. enroll in private school without the aid of a voucher). In earlier studies, when the concern has been to obtain unbiased estimates of treatment impacts, evaluators used Instrumental Variable Analysis, with the lottery result as the exogenous instrument, in order to correct for such biases (Howell et al 2002). The relevant comparison here, though, is not between those students who attended private school and those in public schools who would have gone private if given the chance. Since we are seeking to identify the specific characteristics of schools that may be producing voucher gains, we need to use a straightforward private schooling treatment variable as the source of the effect that needs to be "explained away" by factors causally downstream of the switch to private school.

To estimate the effect of private schooling on test scores, we run the following Ordinary Least Squares (OLS) model:

$$Y_2 = \alpha + \beta_1 P + \beta_2 Y_{0R} + \beta_3 Y_{0M} + \mu$$

Y_2 is each student's total achievement score after two years on the Iowa Test of Basic Skills expressed in NPR points.[1] The total achievement score is a simple average of the math and reading components.[2] P is an indicator variable for whether or not an individual attended a private school during the second year of the evaluation. Y_{0R} and Y_{0M} are the baseline reading and math scores. Baseline test scores are included to adjust for minor differences between the treatment and control groups on achievement on the baseline tests, and to increase the precision of the estimated impacts. The β_1 coefficient therefore represents the estimated impact of switching to private school on student test scores. This model does not include many of the individual-level demographic controls that are commonplace in the sector-effects literature. Elsewhere, researchers have shown that including demographic controls does not influence the estimates of the private schooling treatment effect in the D.C. evaluation (Howell et al 2002).

To identify the effect of private schooling on study participants, we need only estimate the above equation. To identify the proximate causes of this effect, however, more complex analyses are required. In subsequent models, we include additional variables that describe the kinds of public and private schools that students attended in D.C. Should any one (or combination) of these factors explain voucher students' test-score gains, their inclusion in the models should substantially attenuate the observed treatment effect.

IV. Results

The results of our initial search for a specifiœxplanation for voucher achievement gains appear in Tables 1-7. The first row under the column headers in each table simply restates the

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[1] For ease of interpretation, we report impacts in terms of NPR points. The results do not change substantively in any city when using National Curve Equivalents or raw test scores.

[2] Because it is based upon a larger number of test items, the total achievement score is likely to generate more stable estimates than are reading and math scores estimated separately (see Krueger 1999).

average private schooling impact that is produced simply by estimating combined math and reading test score performance based on whether or not the student attended a private school, controlling only for each student's baseline math and reading score. The average treatment gain of about 3.3 National Percentile Rank points, which is statistically significant even beyond $p<.01$, essentially serves as the target to knock down. Should that voucher benefit be largely the result of greater access to some particular educational advantage, the treatment effect coefficient should shrink precipitously in magnitudeand become indistinguishable from 0 upon the introduction of that factor or set of factors into the statistical model.

Our quest begins with school resources. Do the participants in the WSF voucher program appear to be performing better academically largely because they have access to superior academic resources? Apparently, the answer is no. As the Model 1 results suggest, the gains are in no way limited to the small subset of study participants who attended elite private schools, defined as private schools thatare independent of any religious organization. The per-pupil spending level also does not explain the private schooling effect, as evidenced by the results of the Model 2 estimation. The expenditure variable itself is positive and statistically significant, as we might expect. However, controlling for per-pupil expenditures actually doubles the size of the voucher effect. This result suggests that the private schools in D.C. produced test score gains for study participants not because of higher expenditures but *in spite of* lower per-pupil spending than was common in the public schools in the sample. As demonstrated by the Model 3 estimation, the voucher gains cannot be explained by access to more elaborate school facilities or programs. A significant voucher gain remains, and is even much larger in size, controlling for these three important school resource factors.

[Table 1 about here]

If resources fail to explain away voucher gains, might the smaller scale of most private schools be their proximate cause? In this case, clearly the answer is no. As described in Table 2, the gain from the treatment again increases somewhat in magnitude when estimates of school size and class size are factored into the equation. Model 4 includes an explanatory variable for school enrollment, which is positive and statistically significant, suggesting that larger schools actually enhanced the test-score performance of the students in the study. Model 5 adds school data regarding class size as an explanation for voucher gains, and the coefficient suggests that somewhat higher performance is associated with *larger* and not smaller classes, although the coefficient is not statistically significant. More importantly for our purposes, the coefficient for the treatment effect registers about 2 points higher than the threshold effect of 3.3 NPR in both estimations. The opportunity to be educated in smaller schools or classes does not seem to be driving voucher gains. The estimates for the constant term and the baseline test controls were omitted from this and subsequent results tables (but not the regression models that produced them) because they do not contribute to the substance of the analysis and the coefficients for the baseline control variables hardly vary in their magnitude or precision.

[Table 2 about here]

If the scale of the education that voucher students receive is not the source of the voucher achievement gains, then perhaps the students gain access to a more vibrant educational community of involved parents and caring teachers through vouchers. As demonstrated in Table 3, these measures of community fail to explain fully the voucher gains. The index of school-parent communication and parental involvement fails to explain any variation in student achievement, either separately or combined with the Caring Teacher Index. Students who report that their teachers are particularly interested in and attentive to students do tend to achieve more,

and the private schooling effect loses some magnitude and one level of significance when that variable is included in the model; however, a sizable and statistically significant treatment effect remains even after controlling for the existence of more concerned teachers. The source of the voucher effect on test scores must largely lie elsewhere.

[Table 3 about here]

Table 4 presents the results from the examination of whether or not the more orderly environment and stricter discipline experienced by students in private schools explains their achievement gains. Although the introduction of both the order and discipline variables into Model 9 reduces the independent effect of private schooling somewhat, the private schooling variable remains the only statistically significant explanatory variable in the equation. We cannot conclude that a more orderly environment and stricter discipline explain voucher gains.

[Table 4 about here]

What about homework? Do voucher students tend to benefit from more frequent and demanding homework assignments? The results in Table 5 are suggestive that homework might be one source of the private schooling advantage uncovered in our evaluation. In the Model 10 estimation, parental reports regarding the average amounts of daily homework are factored into the equation. More homework is associated with higher achievement downstream, but the relationship is not statistically significant. Moreover, the independent voucher effect sheds less than half a point and remains statistically significant. Because students are a more direct source of information about homework burdens, Model 11 factors their reports into the analysis. Although the treatment effect diminishes by more than a point, it remains statistically significant at least at the lowest level used in this analysis. Moreover, student estimates of their relative

amounts of daily homework are not statistically significant predictors of downstream achievement.

[Table 5 about here]

Do measurable characteristics of the peers with whom students are educated explain the D.C. voucher gains? Here we get our first whiff of a potential smoking gun. As presented in Table 6, the two measures of student body characteristics—percent white students and percent low-income students—demonstrate their expected effects on student achievement. The study students who are in schools with higher percentages of white students tend to perform somewhat better on the follow-up achievement test, all else being equal; whereas, students who are in schools with higher percentages of low-income students may tend to perform somewhat less well, but the effect is statistically indistinguishable from 0. Controlling for these student body characteristics simultaneously reduces the magnitude and precision of the treatment effect by nearly a full point, for the first time rendering it statistically insignificant, although barely so. It appears that exposure to a more advantaged group of peers may be one important cause of the voucher gains that we uncovered in D.C.

[Table 6 about here]

Finally, the possible factors considered here may not, separately, explain voucher gains; however, several of them may provide an adequate explanation for such gains when considered together. Table 7 presents the results of a model estimation that includes factors from multiple schools of thought regarding the drivers of voucher gains that have individually demonstrated promise as possible proximate causes of D.C. voucher gains. Model 14 finally accomplishes what the previous estimations could not: the school characteristic variables in the model shrink the magnitude of the independent private schooling effect by nearly 40% and render it

statistically insignificant clearly beyond conventional confidence levels (p=.15). The higher percentage of white students at D.C. private schools and the dedication of the private school teachers appear to be the primary drivers of the voucher gains that we uncovered. However, the model is highly sensitive to specification changes. If the percent low-income and amount of homework variables (or the controls for baseline reading and math scores) are omitted from the estimation, the independent private schooling effect remains sizable and statistically significant even controlling for the percentage of white students and the caring teacher index.

V. Conclusion

Just as Rome was not built in a day, the answer to the vexing question of what school characteristics or practices might explain the gains evidenced by voucher users is not likely to emerge, decisively, from a preliminary analysis of data from a single voucher experiment. It is left to future analyses to generate more conclusive results regarding the proximate causes of voucher gains. The analysts who conduct such probes should especially target student body demographics and measures of the relative dedication of teachers in their search for why low-income inner-city students appear to benefit academically from private schooling. The amount and difficulty of homework also has emerged from this study as a possible component of the explanation for such gains in D.C. For now, we should draw only tentative conclusions regarding what happens inside the black box to generate voucher gains. We also should encourage more analyses that peer into future voucher black boxes, hopefully with a stronger microscope.

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Table 1: Do School Resources Explain Voucher Gains?

Factor	Base Model	Model 1	Model 2	Model 3
Voucher Gain Alone	**3.27*** (1.25)**	**3.27*** (1.25)**	**3.27*** (1.25)**	**3.27*** (1.25)**
Explanatory Variables				
Private School Effect	3.27*** (1.25)	3.15** (1.29)	6.51*** (2.23)	6.46*** (2.26)
Elite Private Schooling		1.60 (3.92)	-.58 (4.50)	-.50 (4.51)
Per Pupil Expenditure (in thousands)			.82* (.50)	.82* (.50)
School Facility Index				-.02 (.20)
Control Variables				
Baseline Reading Score	.13*** (.02)	.13*** (.02)	.12*** (.02)	.12*** (.02)
Baseline Math Score	.43*** (.03)	.43*** (.03)	.42*** (.03)	.42*** (.03)
Constant	7.96*** (1.09)	7.96*** (1.09)	2.70 (3.40)	2.88 (3.80)
Adjusted R^2	.36	.36	.37	.37
N	730	730	730	730

Observations are of students in grades 3-9 in 2000. Observations weighted to correct for non-response. Missing data dummy variables included in all regressions. Figures are unstandardized regression coefficients with standard errors in parentheses below. * denotes statistical significance beyond p<.10, ** p<.05, *** p<.01, using a two-tailed test.

Table 2: Does the Scale of the Educational Environment Explain Voucher Gains?

Factor	Model 4	Model 5
Voucher Gain Alone	**3.27*** (1.25)**	**3.27*** (1.25)**
Explanatory Variables		
Private School Effect	5.52*** (1.50)	5.24*** (1.55)
School Enrollment (in 00s)	1.16*** (.39)	1.12*** (.39)
Class Size		.19 (.13)
R^2	.37	.37
N	730	730

Observations are of students in grades 3-9 in 2000. Observations weighted to correct for non-response. Missing data dummy variables, constant, and controls for reading and math baseline scores included in all regressions but coefficients omitted from tables. Figures are unstandardized regression coefficients with standard errors in parentheses below. * denotes statistical significance beyond p<.10, ** p<.05, *** p<.01, using a two-tailed test.

Table 3: Do Stronger Communities Explain Voucher Gains?

Factor	Model 6	Model 7
Voucher Gain Alone	**3.27*** (1.25)**	**3.27*** (1.25)**
Explanatory Variables		
Private School Effect	3.30*** (1.2[illegible]	2.75** (1.2)
Community Index	-.04 (.34)	-.03 (.32)
Caring Teacher Index		.74*** (.25)
R^2	.36	.43
N	730	730

Observations are of students in grades 3-9 in 2000. Observations weighted to correct for non-response. Missing data dummy variables, constant, and controls for reading and math baseline scores included in all regressions but coefficients omitted from tables. Figures are unstandardized regression coefficients with standard errors in parentheses below. * denotes statistical significance beyond p<.10, ** p<.05, *** p<.01, using a two-tailed test.

Table 4: Do Order and Discipline Explain Voucher Gains?

Factor	Model 8	Model 9
Voucher Gain Alone	**3.27*** (1.25)**	**3.27*** (1.25)**
Explanatory Variables		
Private School Effect	3.18** (1.25)	2.41** (1.19)
Order Index	.19 (.12)	.14 (.11)
Strict Discipline		.88 (.73)
R^2	.37	.44
N	730	730

Observations are of students in grades 3-9 in 2000. Observations weighted to correct for non-response. Missing data dummy variables, constant, and controls for reading and math baseline scores included in all regressions but coefficients omitted from tables. Figures are unstandardized regression coefficients with standard errors in parentheses below. * denotes statistical significance beyond $p<.10$, ** $p<.05$, *** $p<.01$, using a two-tailed test.

Table 5: Does Homework Explain Voucher Gains?

Factor	Model 10	Model 11
Voucher Gain Alone	3.27*** (1.25)	3.27*** (1.25)
Explanatory Variables		
Private School Effect	2.88** (1.2[illegible])	2.16* (1.2[illegible])
Homework Half-hours (Parent Reports)	.97 (.63)	
Homework Half-hours (Student Reports)		.67 (.4[illegible])
R^2	.37	.42
N	730	730

Observations are of students in grades 3-9 in 2000. Observations weighted to correct for non-response. Missing data dummy variables, constant, and controls for reading and math baseline scores included in all regressions but coefficients omitted from tables. Figures are unstandardized regression coefficients with standard errors in parentheses below. * denotes statistical significance beyond p<.10, ** p<.05, *** p<.01, using a two-tailed test.

Table 6: Do Student Body Demographics Explain Voucher Gains?

Factor	Model 12	Model 13
Voucher Gain Alone	3.27*** (1.25)	3.27*** (1.25)
Explanatory Variables		
Private School Effect	3.02** (1.26)	2.32[@] (1.45)
Percent White Students	.18*** (.06)	.15** (.06)
Percent Low-Income		-.03 (.03)
R^2	.37	.37
N	730	730

Observations are of students in grades 3-9 in 2000. Observations weighted to correct for non-response. Missing data dummy variables, constant, and controls for reading and math baseline scores included in all regressions but coefficients omitted from tables. Figures are unstandardized regression coefficients with standard errors in parentheses below. * denotes statistical significance beyond p<.10, ** p<.05, *** p<.01, using a two-tailed test.

[@] p=.111

Table 7: Do School Demographics, Homework, and Caring Teachings Together Explain Voucher Gains?

Factor	Model 14
Voucher Gain Alone	3.27*** (1.25)
Explanatory Variables	
Private School Effect	2.00 (1.39)
Percent White Students	.12** (.06)
Percent Low-Income	-.00 (.03)
Homework Half-hours (Student Reports)	.56 (.43)
Caring Teacher Index	.72*** (.25)
R^2	.44
N	730

Observations are of students in grades 3-9 in 2000. Observations weighted to correct for non-response. Missing data dummy variables, constant, and controls for reading and math baseline scores included in all regressions but coefficients omitted from tables. Figures are unstandardized regression coefficients with standard errors in parentheses below. * denotes statistical significance beyond p<.10, ** p<.05, *** p<.01, using a two-tailed test.

Appendix for Variables Used in the Analysis

Table A: Descriptive Statistics of Variables for DC Study Participants in 2000

Factor	Mean	Standard Deviation	Minimum	Maximum	Valid N
Dependent Variable:					
Combined Reading & Math Score in 2000	22.9 NPR[1]	19.3 NPR	0 NPR	92.5 NPR	730
Explanatory Variables:					
Private School	31.4%	46.4%	0	1	730
Elite Private School[2]	2 .5%	15.6%	0	1	726
Per Pupil Expenditure[3]	$5,824	$2,071	$395	$18,000	697
School Facility Index[4]	9.3	3.1	1	14	704
White Students	3.5%	12.9%	0%	81%	482
Free/Reduced Lunch	68.4%	28.8%	0%	99%	448
School Enrollment	389.9	205.1	37	1543	527
Class Size	17.3	5.8	4.4	56.7	480
Community Index[5]	5.9	1.8	1	8	698
Caring Teacher Index[6]	15.7	2.8	6	20	439
School Order Index[7]	17.7	4.7	7	21	730
Strict Discipline[8]	2.9	.9	1	4	459
Parent Est. Homework	71 min.	28 min.	0 min.	150 min.	693
Student Est. Homework	85 min.	45 min.	15 min.	165 min.	527
Control Variables:					
Baseline Reading Score	30.3 NPR	27.1 NPR	0 NPR	99 NPR	730
Baseline Math Score	23.1 NPR	21.8 NPR	0 NPR	99 NPR	730

Restricted to students in grades 3-9 in 2000

[1] National Percentile Ranks

[2] Defined as private but unaffiliated with a religious organization

[3] Defined as the regular tuition level at private schools and the district per-pupil expenditure level (net of special education) for public schools, differentiated by neighborhood public and public charter

[4] Based on parental responses regarding the presence or absence of: a computer lab, a library, a gym, a cafeteria, special programs for non-English speakers, individual tutors, special programs for slow learners, special programs for fast learners, child counselors, a nurse's office, a music program, an arts program, an after-school program, and prepared lunches

[5] Based on parental responses to questions regarding whether or not they are: sent mid-term grades, notified when student is disruptive, asked to talk to class about their job, asked to assist in instruction, asked to attend open-houses at school, asked to attend parent/teacher conferences, sent regular notes from the teacher about student, sent a school newsletter

[6] Based on student Likert-scale responses to whether they agree that their teachers: are interested in students, really listen to them, are fair, avoid putting down students, punish cheating when they observe it

[7] Based on parental reports reporting the relative seriousness (very, somewhat, not) of the following disorders: kids destroying property, tardiness, truancy, kids fighting, kids cheating, racial conflict, and guns or other weapons

[8] Based on student Likert-scale responses

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